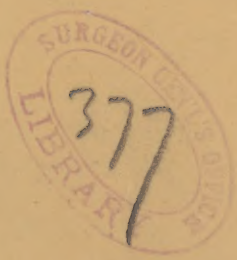
Mason. (J. J.)

Peristaltic arterial action + + 2 ½ article.



Isaac Hays MD
With the author's Compliments.

PERISTALTIC ARTERIAL ACTION.

SECOND ARTICLE.

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In December last I published, in the New York Medical Journal, some experimental and theoretical objections to the views entertained by Légros and Onimus, of Paris, concerning a function which they suppose to be performed by the muscular walls of the arterioles. Their theory is, that the arterial blood-waves dilate, in succession, the portions of the arterial walls with which they come in contact; that these portions or sections contract immediately behind each wave, and thus assist the flow of blood toward the capillaries; that the successive shocks imparted by the waves, as they pass, are the exciting causes of these contractions.[1]

I now offer a reply to one in our own country, who, it seems, has become an advocate of this theory, and not only differs with the writer in opinion, but also makes an attempt to prove that his experiments are unphysiological.

In the first place, as to the objection relating to an electrical point, he errs, in assuming that I regard it as an "unimportant" one. Nothing of the kind was implied in my first paper. At all events, it remains still unattacked, and its importance will be more fully shown in a subsequent article.

Before proceeding further, let it be understood clearly that, in common with everybody, we admit vermicular intes-

[1] This is a correct statement of the theory as given by its originator, Légros, in his "Thèse d'Agrégation," Paris, 1873, p. 83.

tinal action to be a fact. We acknowledge that something in a rabbit's ears is sometimes seen which looks like peristaltic action of the arteries, and we believe that the retinal arteries often act in much the same way when embolism occurs in a single branch. We insist, however, that all such examples as these ought, at the outset, to be excluded from the question under consideration. They are entirely irrelevant facts, have no bearing whatever upon the discussion of the theory as above given, and tend only to confuse. Let us, then, hold fast to the main points at issue, and not think we see a parallel between vermicular action of tubes, like the lacteals upon their inert contents, and that supposed to assist the flow of a wave-like current of blood coming from an organ like the heart.

In my experiments, from which, as it is claimed, no physiological arguments can be drawn, I found that in rabbits, when a stream of milk was forced through the circulatory system of the lower part of the body, the amount of liquid returning by the abdominal vein varied. When the injection was made intermittently, so as to produce waves (as in nature), something in the vessels of the animal offered an obstacle to the flow—an obstacle not offered to a stream coming from continuous pressure. This was determined by noting the ratios between continuous and intermittent streams before and after attaching the canulæ in the vessels of the animal. The details of procedure were all given, and the results (in time) carefully recorded. It was claimed that we ought to have obtained opposite results, were the "peristaltic-action" theory correct. The criticism on these experiments reads as follows: "Now this something" (in the vessels of the animal, which alters the ratio) "is only the greater difficulty with which water (?) passes through the capillaries than it does through the canula." The author of this criticism regards these vessels as standing in the place of a smaller canula, and claims that by experimenting with elastic tubing, terminating in canulæ of different sizes, he has been able to explain my results.

Now, he is simply mistaken as to the facts.

The smaller the canula, the more liquid passes through it

in the same time, when passed intermittently, and compared with an amount passing through a larger canula from the same source and in the same manner.

I have experimented with various lengths of tubing and sizes of canulæ, using always for motor power the irrigateur syringe of Dr. Eguisier, and timing by a metronome the intervals required to fill a vessel of known capacity—ten fluid ounces. These experiments have been conducted with the greatest care, and several times in the presence of other medical gentlemen.

EXPERIMENT No. 1.—Canula, 2 m.m. in calibre. Tube caoutchouc, 2 feet long—same calibre as canula. $\frac{\text{Continuous } 30 \text{ beats.}}{\text{Intermittent } 44 \text{ beats.}}$

EXPERIMENT No. 2.—Canula, 1 m.m. in calibre. Tube caoutchouc, 2 feet long—same calibre as in No. 1. $\frac{\text{Continuous } 66 \text{ beats.}}{\text{Intermittent } 68 \text{ beats.}}$

The proportion is 30 : 44 : : 66 : : 96 + i. e., we should expect to wait for 96 beats, while, in reality, the vessel was filled in 68 beats.

While the use of the metronome is evidently more conducive to accuracy than are measurements of quantity, since the latter method has been adopted by our critic, it was also tried, with the following result:

EXPERIMENT No. 3.—With same apparatus, water measured after flowing ten seconds. Large canula (2 m. m.). $\frac{\text{Continuous } 8 \text{ ounces.}}{\text{Intermittent } 6.5 \text{ ounces.}}$ Small canula (1 m.m.). $\frac{\text{Continuous } 5 \text{ ounces.}}{\text{Intermittent } 5 \text{ ounces.}}$

These results are invariable; they depend upon well-known hydrostatic laws, and furnish additional (though not unexpected) evidence in favor of the position which I have taken.

My next objection was, that peristaltic contractions of an artery, in order to accelerate the flow of blood, must occur synchronously with the beats of the heart. The necessity of such a supposition was, as we see, fully recognized by Légros, for otherwise his theory could not even have been stated in an intelligible form. Now, such rapid contractions of unstriped muscular fibre have no parallel in the organism. "Les fibres lises ont une action moins prompte." To infer the existence of such contractions is purely hypothetical.

Our American opponent goes farther than Légros, and claims that these contractions need not be synchronous with the heart; and, to support his view, he performs some experiments producing intermissions with the ball of a Davidson's syringe, allowing a stream of water to pass through it. "I found," he says, "that a continuous stream would give, when aided by contractions of the ball, from four to thirty per cent. more water in the same time than when unaided." We reply, Imitate Nature. Remove the valves from the syringe, and the results will be exactly reversed. The arteries have no valves. With the valves the flow is accelerated; without them it is retarded.

If the experiments with different-sized canulæ were made with a Davidson's syringe, were the valves in or out? With this instrument connected with the tubing I obtained—

1. Valves in, $\frac{\text{Continuous } 96 \text{ beats.}}{\text{Intermittent } 80 \text{ beats.}}$ 2. Valves out, $\frac{\text{Continuous } 80 \text{ beats.}}{\text{Intermittent } 118 \text{ beats.}}$